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THE USES OF THE PAST

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THE USES OF THE PAST*

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Many years ago (1969) George Stigler wrote an essay which he entitled "Does Economics Have a Useful Past?" Stigler was a learned man with many contributions to the History of Thought that were once required readings for graduate students. He clearly loved the subject. Nevertheless, his essay gave a by-and-large negative answer to the question of its title.[1] My suspicion is that he did not want the answer to be in the affirmative. George Stigler did his useful work in the field of Industrial Organization and Regulation and did it of course with Nobel distinction. The History of Thought was a gentlemanly pursuit, not to be sullied by association with the useful work that one does for a living.

However, that may be, this paper was begun[2] as an attempt to answer his question in the affirmative[3].

Table Manners

Like Stigler, some economists like the history of their field for its own sake. So do some physicists and some musicians. Those of us who do like that sort of thing tend to have little empathy for those who don't. That is certainly one reason for showing no patience with the question "what is history of thought good *for*?" But another reason, of course, might be that we do not know the answer.

The history of economic thought used to be part of the core of graduate studies in economics. Sixty or seventy years ago, it was common for famous teachers like Lionel Robbins or Jacob Viner to approach economic theory by way of its development. Today, of course, this is seen as an archaic way of going about it and has become quite uncommon. The history of thought is still a tolerated

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[1] In his affectionate memoir of Stigler for the National Academies Press, Milton Friedman confirms his friend's love for the subject – and contradicts him on its usefulness: "Intellectual history ... remained a lasting love and provided a seedbed for his scientific work. A deep understanding of the ideas of the great economists of the past gave him a strong foundation on which to build an analysis of contemporary issues." Cf.www.nap.edu/html/biomems/gstigler/html

[2] Also many years ago. An early, partial version was delivered at the History of Economics Society Conference, Harvard University, Cambridge, Mass., June 20-22, 1987. It is said about my countrymen: "The Swede thinks for a long time before he does something rash."

[3] D. McCloskey, "Does the Past Have Useful Economics?" Journal of Economic Literature. (1976) make an affirmative case for economic history, not for the history of economics

subject – in at least a few places – but not one to be studied in order to improve one's understanding of today's economics. Cadets are taught etiquette at West Point and Annapolis. It has a perfectly secure place among what is taught at military schools although the limited value of good table manners in actual combat is more or less clear to all. Our profession is agreed, I think, that an economist should have a smattering of economic thought for the same reason that an officer should have decent table manners. But one does not confuse these things with theory and econometrics or, as the case may be, with tactics and logistics.

This transition in the style of discourse in economics is part of a much broader move away from historical and towards mathematical and statistical modes of reasoning and uses of evidence.[4] A modern macroeconomist, for instance, is more likely to rely on vector autoregressions than on economic history of a more traditional sort. It is best, here, to shun any attempt to characterize and appraise in depth this vast transformation in the ways that we approach social reality. It is obvious, however, that it has been motivated by the ambition to develop economics into a "science" and also that this ambition is widely thought to have been realized. We might inquire, therefore, what attitude scientists usually take to the history of their respective fields.

State of the Art -- or Best Forgotten?

In brief, according to Sir Peter Medawar, "the history of science bores most scientists stiff." (I think we can agree that this is one respect in which economists are often quite successful in aping scientists!) In a paroxysm of good will towards his brethren, Medawar refuses to "dismiss this as cultural barbarism"[5] and chooses instead to regard it as, in itself, posing a scientific question, namely, why this distinctive attitude among people of scientific learning is both "natural and understandable"?[6]

> A scientist's present thoughts and actions (Medawar reflects) are of necessity shaped by what others have done and thought before him: they are the wave-front of a continuous secular process in which The Past does not have a dignified independent existence of its own. Scientific understanding is the integral of a curve of learning; science therefore in some sense comprehends its history within itself.

[4] Those who don't study history, we are told, are condemned to repeat it. That being so, of course, it will eventually become amenable to statistical treatment. Edwin Gay once told Wesley Mitchell that "it would take fifteen or twenty generations more of hard and painstaking work and the accumulation of . . . five hundred years or more [of data] to make statistical deductions from social measurements." (Heaton, 1952, p. 196 quoted in Craver and Leijonhufvud, 1987). That might do it!?

[5] He did not always manage to be as forebearing. Cf., Sir Peter Medawar (1979), pp.xx

[6] Medawar, "Lucky Jim", in his Pluto's Republic, pp. 273-274.

Can we claim for economics that it, similarly, comprehends its history within itself? Or should we accord our past an "independent existence?" (of such dignity as social inquiry might possess)?

Clearly, we have many instances of developments within economics that have, at least for a time, this "scientific" character of the moving wave-front on which young men and women surf to tenure, with all the excitement of "hanging ten" over the integral of learning. Neoclassical growth-models in the late 1950s and early '60s, rational expectation models in the 1970s, and more recently game theory in its latest incarnation, real business cycle theory or endogenous growth models are examples of such waves, where the relevant literature is the latest batch of working papers off the Web and where references to anything 10 years old would only be made if the occasion is deemed to require a demonstration of good table manners. The number of examples, moreover, could be multiplied and most of them would belong to recent decades. The developments of this kind that come most easily to mind seem all to belong either to mathematical economics of a fairly "pure" kind or to econometric method. It is not always obvious that all these waves have done much for "Our Knowledge of the External World" (Russell, 1914). But whether this is so or not, it is clear that not everything that goes on in economics cumulates in this nice fashion.

Not every field of learning can claim to "comprehend its history within itself." For the current state of the art to be the "integral of past learning" in Medawar's sense, the collective learning process must be one that remembers everything of value and forgets only the errors and the false leads. But this requires the recognized capability to decide what is correct or true and what is in error or false. These decisions, moreover, must compel general assent. Once an answer is arrived at, it must be generally agreed to be the answer. The field must be one in which answers kill questions so definitively that the sense of alternative possibilities disappears.[7] What people used to think about the dead question in its lifetime will not be of current relevance to the field. To the extent that their notions were correct, they survive in the current answer; to the extent that they were false, they remain useless.

Decision Trees

A science, or a subfield within it, may come to approximate these conditions because of its positive successes. But two other mechanisms that are not so nice will also be at work. First, the people in the field agree that certain questions, which they would have a hard time deciding, are somebody else's responsibility. So economics among the social sciences, like physics among the natural sciences, had first pick of problems and left the really hard ones, on which their methods did

[7] The answers that kill I have from. Dennis Wrong's (1961)"The Oversocialized Conception of Man in Modern Sociology". This fine essay deals with the counterpart in sociology to the overrational conception of man in economics.

not give them a firm grip, for the younger sister disciplines to deal with as best they might. Second, the insiders to the field will agree to exclude some people who refuse to assent to the manner in which certain important questions have been settled. Both the exclusion of undecidable questions from the field of inquiry and the exclusion of undecided people from the professional group help to achieve collective concentration and intensive interaction within the group. You cannot worry about everything -- or even less everyone's opinions about everything -- and still do good work on something at the same time.

The picture of a science without an "Independent Past" that emerges from these considerations resembles, I would suggest, that of a Kuhnian case of "normal science" or that of a healthy Lakatosian "research program." We should expect to find the ahistorical attitude among a group of scientists busily solving puzzles within an agreed-upon paradigm (or proceding in line with the positive heuristic of a program's hard core). Preoccupation with the past is then a diversion and a luxury. When things are going well, it is full steam ahead! Why look back? The people who are doing just swimmingly in the mainstream are unlikely to make a good audience for doctrine history.

These reflections will not qualify as a philosophical theory of when the scientific past is irrelevant. Nonetheless, they offer some suggestions about when scientists might find the history of their field relevant and useful to current inquiry. One suggestion is to look for situations when a research program has bogged down, when anomalies have cropped up that cannot be reduced to or converted into ordinary puzzles within the paradigm. Another is to look for cases in which three conditions seem to be met:

a) certain central questions cannot be decided in a way that commands assent,
b) the (for the time being) undecidable questions cannot very well be left for somebody else to worry about, and
c) the people who withhold their assent from some popular suggested answer cannot be ignored or excommunicated.

This is the kind of situation where we may expect to see competing schools of thought and controversies that go on and on and on without a clear resolution being achieved.

Economists are wont to reduce everything to choices. Economics itself develops through the choices that economists make. To use the past for present purposes, we should see the history of the field as sequences of decisions, of choices, leading up to the present. Imagine a huge decision tree, with its roots back in the time of Aristotle, and with the present generation of economists -- not all of them birds of a feather! -- twittering away at each other from the topmost twigs and branches. The branching occurs at points where economists have parted company, where *problematic*

decisions had to be made but could not be made so as to command universal assent. The two branches need not be of equal strength at all; in many cases, universal agreement is eventually reached *ex post* so that one branch eventually dies and falls away. The oldest part of the tree is, perhaps, just the naked trunk; but the sap still runs in some surprising places.

If you want to translate Medawar's image of science into my decision tree metaphor, you will have to imagine his sciences as fir trees -- with physics, surely, as the redwood -- majestic things with tall, straight trunks and with live branches only at the very top. Economics, in contrast, would come out as a rather tangled, ill-pruned shrub of some sort, but perhaps nonetheless a decorative one. (As the Kingston Trio sang log ago: "Lemon tree very pretty, and the lemon flowers sweet.")[8]

A problematic question – which is to say a *real* question – is the very opposite to the kind that we pose to students in examinations. Examination questions have to have perfectly decideable answers or the chore of grading would be even worse than it is. The branching occurs over issues that are hard to decide: Is one approach to be preferred to another? Is a basic metaphor or analogy a good one or a misleading one? Does the substitution of an easy, solvable problem for a hard and presently insoluble one get you closer to an answer or lose the essence of the question? Between two models, known to be false -- but, perhaps, the only ones we've got -- which one will come closer to the truth? Every branch of today's economics, including those most insistent on rigor, has a large number of decisions of this sort below it in the tree.

I wish to maintain that knowledge of the past, when the past is understood as such a decision-tree, can be quite useful to the economist working on present questions at today's frontier. How much doctrine history our students "ought to learn," I do not know. I do believe they would be better off if they at least understood why and how it can be useful. But this way of looking at the past for its usefulness is problematic. In particular, its legitimacy from a historical standpoint may seem questionable.

Uses and Misuses

Not all the "Uses of the Past" derive simply and straightforwardly from "telling it like it was." Historians know this in their bones and are always on their watch for people who, for some ulterior purpose of their own, tell stories about the past "like it was *not*." In economics, today, black marks are seldom awarded for just making it up as it suits you. This is unfortunate because in a science, or in a discipline like economics, our understanding of the past shapes our understanding of the tasks presently before us and suggest how they are to be approached. Unavoidably, ulterior

[8] A somewhat unfortunate association. The song continued: “But the fruit of the poor lemon is impossible to eat”!

purposes are always lurking about, when economists talk about their past. Adherence to a particular view of the past limits the research options presently regarded as open; ordinarily, this is desirable since, by the same token, it focuses research efforts in directions that have proved workable in the past. As long as "normal" progress continues to be made in these established directions, there is no need to reexamine the past -- and it is then more than likely that increasingly simplistic notions of the development of the field will gain widespread acceptance. Indeed, when a group agrees on what is "good economics," on what the "relevant" questions are, and on how to go about answering them, a version of the past that is more legend than history may serve its scientifically ideological purposes very well.

Things begin to look different if and when the workable vein runs out or, to change the metaphor, when the road that took you to the "frontier of the field" ends in a swamp or in a blind alley. A lot of them do. Our fads run out and we do get stuck occasionally. Reactions to finding yourself in a cul-de-sac differ. Tenured professors might often be content to accommodate themselves to it, spend their time tidying up the place, putting in a few modern conveniences, and generally improving the neighborhood. Braver souls will want out and see a tremendous leap of the creative imagination as the only way out -- a prescription, however, that will leave ordinary mortals just climbing the walls. Another way to go is to backtrack. Back there, in the past, there were forks in the road and it is possible, even plausible, that some roads were more passable than the one that looked most promising at the time. At this point, a mental map of the road network behind the frontier becomes essential.

Alternative Futures – at Junctures in the Past

Backtracking will only help, however, if one can regain some sense of the "alternative futures" that once were -- although they never happened.[9]

Backtracking from the present to put essentially anachronistic questions to long dead authors is a strange way of dealing with the past. It seems a very different, a suspiciously different, enterprise from that of writing "proper" doctrine history. The novelist and essayist, Leo Rosten (1970) tells of a wandering story-teller who, finding his veracity challenged, replied with dignity:

> *But much is true that doth not happen, and much that happens gaineth not in verity because of blind occurance.*

This sounds a rather different note from the Rankean "wie es eigentlich gewesen ist." But the theorist found wandering the byways of the history of the subject is apt to have the story-teller's

[9] This problem is pursued in greater depth in my “Time in Theory and History” (1986).

attitude for, probably, he is first and foremost *looking for a way out*. Those who live in the place and like it will find these tourists on their strange excursions in search of a "usable past" rather offensive (even as they help pay for the upkeep of local monuments). What kind of mutual accommodation can one hope for between these users and the producers of the historical record?

All history writing demands the exercise of selectivity among events, but the user's retrospective selection on the basis of presumed relevance to current work will seem outright ahistorical with its barely disguised anachronisms and its counterfactual speculations about the possible truths that "never happened." But there is, I would argue, at least one accepted and much practiced genre of the history of thought that is *worse*, even though it maintains the outward decency of recounting the past chronologically rather than diving into it retrospectively. This is the history of economics told as "progress," as a story of the *right* decisions, from earliest days to our own times, each decision tagged with the name of the hero who made it -- and all of it leading straight to the branch of the decision-tree on which the author has been comfortably perched all along. Superficial appearances to the contrary, this kind of Whig history[10] writing is just as teleological as the backtracking from some current impasse which theorists might undertake. It may have a certain propaganda value, perhaps, because it suggests that economics has grown as a redwood after all. But it is a useless genre because it is predicated on the view that the present state of the art is *unproblematic* . This makes it useless for, as we have seen, it is *when the present is unproblematic that the past is uninteresting*. History of thought told as progress is history from which the usable past has been deleted.

In a talk I heard him give years ago, Donald Winch admitted with some regret (but no apparent remorse) that the firmly "anti-teleological" intellectual history that he practices sometimes reads like a "chronicle of failures." But this kind of effort may have a better chance than "history-as-progress" to unearth a past that is usable to someone. For economists who take an interest in the past will want to know about the alternatives that were rejected or followed only by minorities.

Prescient Masters or Sleepwalkers?

Still, the historian and the user of history approach the past with different intentions. The historian's recreated past, therefore, will not be the same as the theorist's useable past. The great economists of the past perceived their problems, the alternative approaches before them, and the criteria by which to choose among these alternatives differently from how they appear in retrospect. In order to understand how economics actually evolved one must attempt to recreate the old debates

[10] Paul Samuelson (1987, 1988) has made himself the spokesman for Whig history in economics. But this is Paul Samuelson the "Let Us Now Praise Famous Men" after-dinner-speaker speaking. His research in the history of thought debates the choices made by his predecessors -- and, as often as not, finds them taking the wrong fork.

as they were understood by the participants. Hindsight is not always such a benefit in this context. It can be misleading.

Jorge Luis Borges identifies one danger in one of those paradoxical epigrams strewn with such calculated casualness throughout his writings: "Every writer *creates* his own precursors,"[11] he claims. This is a warning that might be addressed particularly to historians of ideas.

Consider some *roles* in the history of a discipline and let's locate them in relation to what is generally seen in retrospect to have been an important fork in the road. One is that of the recognized hero who identifies the fork, dramatizes the necessity of making a choice, and persuades the profession, or at least a major school within it, to follow his chosen road. Another is the writer or scientist who, ahead of others, takes the right turn at the fork, but does not succeed in forcing the choice on his contemporaries or making them follow. A third is the one writing before the fork came into view when things were still undecided and did not have to be decided.

The hunt for precursors who lacked decisive influence in their own time is almost a distinct *genre* within the history of ideas. But it is a difficult *genre*. Finding a hitherto obscure writer who has many "prescient" and intelligent things to say is such a delight that it is hard not to exaggerate their importance -- or the significance of the hunt itself.

The writer's work, clarifies Borges, "modifies our conception of the past, as it will modify the future." We see the past in the light of the works that have influenced us the most and that light, shining back in time, picks ideas, arguments, and hypotheses out of the shadows and will, sometimes, lift an entire *ouevre* out of obscurity[12]. This creation of precursors is not often as willfully capricious as when Keynes, toward the end of the General Theory, chose to spotlight Mandeville, Malthus, Major Douglas, and Silvio Gessell. But it can easily distort our vision of how our field has evolved nonetheless.

The hindsight that economists interested in the historical roots of current problems bring to the past is likely to illuminate the Old Masters only in parts, creating *ciaroscura* portraits, where much of the picture is left in deep shadow. In some cases, better so. Some of the giants of the past belong among the "sleepwalkers" of Arthur Koestler,[13] that is, among writers who changed the course of intellectual history but did not themselves anticipate at all clearly the future ramifications of their new ideas. Their recognized contributions may then be surrounded by a penumbra of strange speculations[14] which, indeed, do not pose a useful past for the present-day researcher.

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[11] Jorge Luis Borges, "Kafka and His Precursors," in his Labyrinths

[12] According to Stigler's Law of Eponyms, "No finding is ever named for its original discoverer." How unjust! Or is it? May it not be, rather, that Borges' paradox is the deeper, underlying explanation for that of Stigler's?

[13] Arthur Koestler, The Sleepwalkers, New York: MacMillan 1968.

[14] Koestler was particularly fascinated by Kepler whose model of the motions of the planets was a byproduct of his passionate search for the harmony of the spheres.

But in other cases the retrospective spotlight picks out only what today seems trite and obvious, missing the grander conception – and leaving the "alternative futures "entirely in the dark.

Neoclassical Theory and Neoclassical Models

There is a misconception about, which one encounters among one's students, namely, that neoclassical economics was always about optimizing and equilibrium. But of course they are projecting their modern training back into the past. Up at least through the 1950's, neoclassical economists always distinguished between *static* and *dynamic* theory. Dynamics referred, on the one hand, to the adaptation of individuals and, on the other, to the market process whereby they collectively groped towards equilibrium. Equilibria were understood as the point-attractors of these adaptive processes. Static theory dealt with the properties of these attractors. The statics was the only part of this older neoclassical theory that could be formalized – which naturally helped its transmission down through student generations (Leijonhufvud 2004). What is called dynamic theory today is just the intertemporal generalization of the old static theory.[15] The problem is that when the older neoclassicals are seen as nothing more than the quaint forerunners of today's neoclassical economics, the larger part of their theory has disappeared from view into the shadows.

Part of the problem is that in today's usage 'model' and 'theory' has come quite generally to be used as interchangeable terms. What is not explicitly modeled does not qualify as theory. Precision in communicating ideas requires formalization. That is all very well, but theorists today seem awfully close to the conceit that statements made in a natural language – such as English -- are not really understandable (Leijonhufvud 1997). The tendency to treat model and theory as synonyms has resulted in the curious usage you sometimes hear among economists to the effect that this or that "may be true in theory" but the "real world" is different. It is hard to imagine physicists or biologists or geologists talking like that! Only economists do, I think, and they do so, of course, because they frequently do not believe their models.

Earlier generations certainly regarded theories as *systems of beliefs*, albeit hypothetical beliefs, about the "real world" and models as constructions that could clarify the logical coherence of parts or aspects of a theory – but not as constituting the whole of it. There were many things they believed to be true about the economy but did not have the mathematical tools to model explicitly. This inability to formalize essential aspects of theory put severe limits on how far such theory could be developed and, in certain cases at least, was to prove an eventually fatal handicap (Leijonhufvud

[15] Franco Modigliani, in his teaching, always referred to intertemporal equilibrium constructions as "meta-static." But that was more than 40 years ago, when the older usage of statics and dynamics was still common coinage.

2006a). But it remains true nonetheless that you get poor value out of reading the giants of the past simply looking for the antecedents of today's models.

By reading them through the glasses of present day theory, we tend also to succumb to the Whig fallacy of "history as progress". The trouble is not only that by judging a writer by today's standards we often misjudge. The more important point is that by reading him as a more or less perspicacious forerunner, we may put that writer on the entirely wrong branch of the tree.

The Cambridge Branch

The Cambridge theoretical tradition has receded deep into the dark shawdows cast by the light of modern general equilibrium theory. But in it, I will maintain, dwells a Usable Past. It is usable, moreover, precisely because the general equilibrium line has run out and, in the end, has proven incapable of saying anything uself about those states of extreme monetary instability which were the reasons for the emergence of macroeconomics as a separate subdiscipline in the first place. To find that usable past, however, it is first necessary to disabuse oneself of the notion that Marshall must have been a muddled Slutsky and Keynes a fix-price Walras.

Marshall did not build from choice theory, did not represent decisions as solutions to constrained optimization problems, and was far too reasonable a man to spend much effort trying to rationalize rationality (Leijonhufvud 2006a). By today's standards, I suppose, he might be judged as having had no microfoundations for his microtheory. How then did he proceed?

Our peculiar practice of drawing supply-and-demand diagrams with quantity on the horizontal and price on the vertical axis we have inherited from Marshall. In his case, however, it was *not* peculiar because he drew demand-price and supply-price schedules as functions of quantity in that space. These, of course, are not loci of optimal points. They give rise to simple adaptive routines, such as: "when demand price exceeds the market price increase consumption of the good"; or "when market price exceeds the supply price, increase production." As a reminder that the theory is dynamic – in the *old* sense of the word – let me refer to these behavioral rules as "Marshall's Laws of Motion."

Marshall has been much criticized by Samuelson and others for postulating the marginal utility of money to be constant. But consider its role in his theory of consumer demand. Marshall's demand curve for tea, for example, is obtained by dividing the marginal utility schedule for pots of tea by the marginal utility of a shilling. The result is a locus of points which have the dimension "number of shillings per pot of tea." Then the consumer's rule is to buy tea until his demand price comes to equal the market price.

This demand schedule is neither Slutsky-compensated nor -uncompensated.[16] While this may be confusing to anyone trying to read optimal choice theory into Marshall, his construction of demand is attractive as a piece of behavioral economics. It envisages a consumer who, knowing the value of money in utils to himself, can go shopping and decide one good at a time whether it is worth spending more money on it. This consumer does not need to calculate an n-dimensional Lagrangean before making any purchase at all. It is sufficient that he has a good idea of the opportunity cost of spending the money in his pocket.

If the agents does know the value of money to himself, his adaptive process will converge to a local optimum mathematically indistinguishable from the one we get from Slutsky or Hicks & Allen. This, of course, is why Marshallian theory has been almost universally confused with microtheory based on optimal choice. The static models may look the same but the theories are distinct. Marshall's theory, rightly understood, does not invite the kind of generalizations to infinite nT-dimensional spaces that are so tempting to make in models based on choice theory. It applies in a region of commodity space that is "local" in space, in time, and in the memory of the agent. It certainly does not apply in regions where no markets exist..

It is possible, on the other hand, to tease some interesting implications from Marshall that we do not get from Slutsky. Consider, for example, the consumer's problem in a high inflation when he cannot know his marginal utility of a unit of money with any confidence. Then his heuristic routine will fail him. Trying to obey Marshall's Law of Motion, he will not end up at the optimal point on his budget constraint.[17] This application of the theory explains two things. First, that the standard measures underestimate the welfare costs of inflation because they are based on a theory that denies the behavioral relevance of cognitive issues. Second, that ordinary people find inflation *frustrating* in ways that economists are trained *not* to understand. In the terms favored by Andy Clark (1997), people need the cognitive "scaffolding" that a stable money provides.[18]

Does this exercise read too much into Marshall? I do not think so. Tiziano Raffaelli has shown us that Marshall had thought deeply about cognition and behavior and had done so, moreover, before he began to construct his economic theoretical edifice. Raffaelli's admirable book (2003) has brought a Usable Past out of the shadows.[19] It is only in recent years that cognitive and

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[16] This simple observation allows me to make a once-in-a-lifetime statement, namely, that I believe John Hicks, Paul Samuelson, and Milton Friedman have all been wrong! -- albeit in the somewhat less than earth-shaking matter of Marshall's theory of demand

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[17] Cf. Heymann and Leijonhufvud (1995), pp. 156-60.

[18] The final conclusion of Heymann's and my book was that high inflations destroy the institutions on which people have to rely in order to come at all close to optimizing.

[19] Acknowledge works of Groenewegen and Brian Loasby here.

behavioral economists have caught up with the young Marshall. And the profession at large is as yet not quite ready to embrace the kind of work being done in these fields.

Searching for What Did Not Happen

People who have had their fill of the endless debate over Keynes's contribution often satirize it as a battle of claim and counterclaims over "what Keynes really meant." And, indeed, if this was doctrine history "for real," what else could it be about? But, viewed from my decision-tree perspective, "what did X really mean?" turns out, more often than not, to be a bad question, a starting point for a rather pointless inquiry. This is particularly likely to be so if the context is some quarrel over who has the best claim to be a true latter-day X-ian. Joan Robinson, James Tobin, Franco Modigliani, Lawrence Klein, Robert Clower or Paul Davidson -- all these later Keynesians are considerably higher up among the branches of the decision tree than Keynes, separated from him by a number of theoretical choices made since his day either by they themselves or by others in the tradition to which they belong.

To approach the *General Theory* with the question "what did he really mean?", therefore, can easily result in an attempt to force Keynes' text to reveal decisions among alternatives that he may never have perceived or, in any case, did not feel obligated to choose between. On many points it is, of course, possible to establish intended meaning -- we would be completely at sea otherwise -- but, on other points, pressing the question will end up attributing some later writer's decisions to Keynes. In doing so, one narrows the perceived range of possible futures that his work once had.

For someone climbing back down the decision tree into the past in order to look for alternative paths to the top, questions about "what Keynes did *not* say" are, at least to begin with, more immediately important that what deeper meaning might be inferred from what he did say. One is trying to regain the options subsequently foreclosed by later writers. In the case of the work I did so many years ago[20], climbing down some thirty years – that have now turned into seventy years! – to the *General Theory* meant negating a great many assumptions or propositions that had become standard properties of Keynesian models in the interim. Keynes did *not* assume rigid wages. He did *not* assume households to suffer from money illusion in the labor market (but being at the same time free of it in the goods market). He did *not* believe investment to be interest-inelastic. *Others* later chose to abandon the speculative demand for money. A *later* generation formed the judgement

[20] My 1968 book tried to deal with a number of then current theoretical issues. In the course of working on it, I repeatedly found it helpful to backtrack to Keynes himself bypassing many later eminent Keynesians. I did not intend to do history of thought and , in the end, was rather puzzled by the kind of history I had been doing. To which this essay bears witness.

that the Pigou-effect disposes of intertemporal coordination failures as an explanation for persistent unemployment. And so on.

"But much is true that did not happen." Or so one hopes. If current theory ("by blind occurance") fails to make progress on some set of questions, it is most likely because someone took the wrong turn somewhere in the past. If the *General Theory* (to take the case in point) dictated this one future that actually did happen, *if properly understood it left no degrees of freedom*, so that the Keynesian economics of the 1960's[21] was "what Keynes really meant" -- well, then, Keynesian economics and the economics of Keynes must share the same fate. Either there were other possible futures inherent in the *General Theory*, or the book is dead.

The first task in finding the usable past, I think, is to recover the indeterminacy, the alternative futures, the questions that were not quite killed by the answers. The historian will want to go on to establish what future would have been most consonant with the author's original intentions, beliefs, or hunches. The user, on the other hand, will want to speculate on what alternative future might have been the most promising. With regard to that question, he or she may have different ideas now from those the author had then.

Keynes

The next story I have told far too many times, so I will try to make it brief.

The point of departure is Marshall. In his day, no one – with the possible exception of Wicksell – doubted that, if everyone (including workers) obeyed Marshall's laws of motion, the economy would surely move towards a full employment equilibrium. The only requirement was "flexibility" on all hands. (Sounds familiar?)

Keynes, we know, set great store by conventions – for other people. But the Great Depression made him challenge conventional wisdom in this matter. He did so, of course, from within the Cambridge tradition. Marshall had relied heavily on what he called his "continuity principle." *Natura non facit saltum*. He may have thought that in a world where change was always gradual, his "gradient crawling" agents would always be able to adapt successfully. Keynes first departure from Marshall was in effect a departure from the continuity principle: he argued that the state of long-term expectation, and with it the marginal efficiency of capital, could shift both abruptly and drastically[22]. However, he also thought that the expectations underlying the long rate

[21] -- or the New Keynesian Economics of more recent date.

[22] Arthur Burns (195x) was, to my knowledge, the only one to perceive this argument of Keynes as a significant departure from tradition and to challenge it. The disaggregated data collected by the National Bureau of Economic Research, first under Wesley Mitchell and then Burns, showed that activity levels of industries did not fluctuate at all synchronously over the business cycle. NBER data did not show "nature jumping."

of interest were incredibly slow to change. Between the entrepreneurial hares and the tortoise investors, the coordination of saving and investment could easily go wrong.

His second departure from Marshall – and from conventional wisdom – was more far-reaching. Flexibility of prices, he argued, could not always guarantee convergence to full employment equilibrium. Indeed, too much flexibility could make matters worse. He had discovered "effective demand failures." Current saving is not an effective demand for future consumption, nor is the offer of labor services by itself an effective demand for current consumption. Consequently, it is possible to have excess supply in some markets without a corresponding effective excess demand elsewhere. If then pricing and production decisions respond, not to what Clower called "notional" demands, but only to effective excess demands, the economy will, under certain conditions, not find the full employment attractor but settle into a state of large-scale – and, in part "involuntary" -- unemployment.[23]

The market values of effective excess demands do not in general sum to zero. Say's Law, as Keynes used the term, does not hold. This is fundamental to Keynes's economics because the case for stablization policy, understood as aggregate demand management, rests ultimately on the proposition that "supply does *not* create its own demand."

The particular problems with self-regulation that Keynes identified are not as ever- present as he thought. The miseries of the Great Depression must have seemed endless to his generation. So, it must be confessed, he exaggerated the deficiencies of the capitalist economy. On the other hand, it must also be said, his theory certainly did not exhaust the troubles that such economy can stumble into.

Model versus Theory Again

Having praised the theory, let me turn to the model. Once more, begin with Marshall. In Marshall's model of a single market in the short run, we have two laws of motion operating, one for price and one for output.[24] The interaction could easily be non-linear. Marshall "tamed" the model by assuming prices moved to clear the market on the market day, and that output found its point

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It is interesting to note that Burns had been the young Milton Friedman's teacher at Rutgers University. Subsequently, of course, Friedman had a long association with NBER. Shocks to the marginal efficiency of capital were the standard disturbance assumed in Keynesian economics, but were not a concern in Friedmanian Monetarism.
Burns, I assume, must have been a Marshallian. Friedman often expressed his preference for Marshall over Walras and Walras' later-day acolytes.

[23] "Involuntary unemployment" as Keynes used the term does not signify interference with or constraint upon the exercise of individual choice. Today, it would be better translated as "unemployment that is not the result of intentional interference with markets."

[24] Things could be worse, of course: The model might have contained producer and consumer inventories as well, for example.

attractor in the short run. But he knew that his statics could not be completely trusted and said so many times.

Keynes' problem was of course infinitely worse. He was trying to deal with a system with multiple markets with different patterns of equilibrating adjustments, with expectations being revised at different speeds, with a monetary structure where "goods buy money and money buys goods but goods do not buy goods" – and, consequently, with effective demand failures. Surely, such a system threatened to be a dynamical nightmare. The dynamics of it was completely beyond what could be done mathematically – and he was trying to find a *static* short-run equilibrium version of it! To find a manageable static model that would capture the essence of the theory, he had to *talk his way* through the system dynamics that were mathematically intractable. It is far from obvious that a short-run attractor with the requisite properties should exist – although Keynes eventually thought he had found one. And all the users of IS-LM through several decades obviously thought so too. But there was confusion from the start, for example, in the debate over what the equality of saving and investment meant. And casting Keynes' theory in the IS-LM mold eventually produced the fatuous conclusion that he had explained unemployment by invoking wages that were too high and rigid downwards.

Robert Lucas (1980) has argued that the History of Economic Thought is largely a story of technical advances in modelling. His is a technocratic version of Whig history, but he has an important message nonetheless. The story of Keynesian economics shows the obverse of the Lucas thesis. An extraordinarily important theoretical insight was misunderstood and eventually lost due to the lack of a mathematical technology capable of communicating it correctly and to develop it further. To young economists with modern theory it is alll but incomprehensible (Leijonhufvud 1998).

The End of Cambridge?

Two more forks in the tree bring us up to the present where Keynesian economics and with it the Cambridge tradition has lost all influence in economics.

The first fork came with the introduction of the the natural rate of unemployment or NAIRU. In theories with the NAIRU property, unemployment in excess of the natural rate is explained simply by lags in the adjustment of wages. It was accepted by professed Keynesians because Keynesianism had come to mean just that. But natural rate theory meant also the reintroduction of Say's Law into macroeconomics.. Supply creates its own demand in NAIRU models. There was some resistance to the logical conclusion from this premise but it eventually had to be accepted that

these models, therefore, cannot provide a rationale for aggregate demand policies. Macropolicy – if any .– can only be supply-side policy.

The second fork came when Rational Expectations theory was extended from perfect stochastic forecasting of nominal values one short period ahead to perfect forecasting of real magnitudes 'until Kingdom come'. The coordination of saving and investment is then no longer a problem and, ultimately, saving and investment decisions are made by the same person – your friend and mine, the representative agent!

So we are left with a macrotheory with no intertemporal coordination problem, no effective demand failures, some labor market inflexibilities, no aggregate demand problems, and only supply side policies. (What good is it? you may ask. You may ask -- but it is not for me to answer).

There remains today, however, a future that did not happen. Agent-based computational economics does provide the technology for the want of which the Cambridge tradition came undone. But it will have to be people younger than me to grasp the opportunities that this technology offers!

Conclusion

The argument of this essay can be put in today's parlance: If you cannot think outside the box, how do you know you're in the right box? It is all very well to know your way around a broad class of models and be able to play with the assumptions and get more or less new results. But this only amounts to rearranging the furniture in the familiar box. It does not give you a view from the outside.

For that wider perspective and the measure of intellectual independence that it brings, the History of Thought is your best bet. At the very least, it will tell you how you got into that box in the first place. If successive decisions have narrowed the options to the point where the box gets claustrophobic, backtracking reopens lost vistas. Acquaintance with the original thinkers back there in the Past can be a great help. There were probably more of them in the Past than we have in the Present.[25]

So you see, my argument is that the history of our subject is not about table manners after all. Properly understood, it is all about strategy and tactics. To be kept alive, it had better be taught that way.

[25] In any case they often make better company than the latest batch of working papers. But this, of course, is the case for the History of Thought that I was trying quite hard *not* to make!

References

Borges, Jorge Luis. 1981. "Kafka and His Precursors," in his **Labyrinths**, Bungay, Suffolk: Penguin.

Burns, Arthur F .1954. **Frontiers of Economic Knowledge**, Princeton: Princeton University Press.

Clark, Andy. 1997. **Being There: Putting Brain, Body, and World Together Again**, Cambridge, Mass.: MIT Press.

Craver, Earlene and Axel Leijonhufvud. 1987. "Economics in America: the Continental influence", **History of Political Economy**, 19:2, pp. 173-182.

Friedman, Milton. "George Stigler" Cf.www.nap.edu/html/biomems/gstigler/html

Heaton, Herbert. 1952. **A Scholar in Action: Edwin F. Gay,** Cambridge. Mass.: Harvard University Press.

Heymann, Daniel and Axel Leijonhufvud. 1995. **High Inflation**, Oxford: Oxford University Press.

Keynes, John Maynard. 1936. **The General Theory of Employment, Interest and Money**, London: MacMillan.

Koestler, Arthur. 1968. **The Sleepwalkers**, New York: MacMillan.

Leijonhufvud, Axel. 1968. **On Keynesian Economics and the Economics of Keynes**, New York: Oxford University Press.

----------. 1986. "Time in Theory and History – Or Why I am Not a Historian," **Agricultural History**, Winter, repr, in Leijonhufvud, **Macroeconomic Instability and Coordination: Selected Essay**, Cheltenham: Edward Elgar 2000.

----------. 1997. "Models and Theories," **Journal of Economic Methodology**, 4:2, pp. 193-198.

----------. 1998. "Mr Keynes and the Moderns", **European Journal of the History of Economic Thought**. Also in Luigi Pasinetti and Bertram Schefold, eds., **The Impact of Keynes on Economics** in the 20th Century, Cheltenham: Edward Elgar, 1998.

----------. 2004."The Metamorphoses of Neoclassical Economics,: in Michel Bellet, Sandye Gloria-Palermo, Abdallah Zouache, eds., **Evolution of the Market Process: Austrian and Swedish Economics**, London: Routledge.

----------. 2006a "Marshall on Market Adjustment," in **Elgar Companion to Alfred Marshall**, ed. by T. Raffaelli, M. Dardi and G. Becattini, forthcoming

---------- 2006b. "Keynes as a Marshallian," in **Cambridge Companion to Keynes**, ed. by Roger Backhouse and Bradley Bateman, forthcoming

Lucas, Robert E. Jr. 1980. "Methods and Problems in Business Cycle Theory," **Journal of Money, Credit, and Banking**, 12, November, pp. 696-715.

McCloskey, Donald N. 1976. "Does the Past Have Useful Economics?" **Journal of Economic Literature,** 14: 434-61, June.

Medawar, Sir Peter. 1984. "Lucky Jim", in his **Pluto's Republic**, New York: Oxford University Press.

----------. 1979. **Advice to a Young Scientist**, New York: Basic Books.

Raffaeli, Tiziano. 2003. **Marshall's Evolutionary Economics**, London: Routledge

Rosten, Leo. 1970. **People I Have Known, Loved, or Admired**, McGraw-Hill.

Russell, Bertrand (1914) **Our Knowledge of the External World**, London: Routledge.

Samuelson, Paul A. 1987. "Out of the Closet: A Program for the Whig History of Economic Science," **History of Economics Society Bulletin**, 9:1, pp. 51-60.

----------. 1988. "Keeping Whig History Honest," **History of Economics Society Bulletin**, 10: 2, pp. 161-67.

Stigler, George J. 1969. "Does Economics Have a Useful Past?" **History of Political Economy**, 1:2, Fall, pp. 217-230.

Wrong, Dennis H. 1961. "The Oversocialized Conception of Man in Modern Sociology," **American Sociological Review**, April, 26:2, pp. 183-196.

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